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RECENT SUGGESTIONS FOR THE RECONCILIATION OF THEORIES OF PROBABILITY

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1. Introduction

For quite some time the different theories of probability appeared to be in hopeless conflict with one another. Now, however, it seems as though an effort to reconciliate the opposing ideas might be opportune. A recent paper [1] of M. G. Kendall is precisely entitled "On the Reconciliation of Theories of Probability." The same purpose is expressed by L. J. Savage in his paper [2] presented at the Boulder Meeting of the Econometric Society.

Even if efforts of this kind appear to some as ineffective or premature, there are many indications that such discussions will present themselves in the near future. The fact is that different concepts connected with probability have been developed and clarified. Different points of view have proved their vitality by appearing time and time again, in spite of the torment of having to overcome misconstructions. However, the present state of affairs seems to be more fluid than in the past. In order to stabilize it, the old discussions resulting from misunderstandings between the different theories should be replaced by new ones which recognize the reciprocal positions of these theories, so that each standpoint is correctly understood.

The purpose of this paper is to state my opinion about the points of view expressed in some recent contributions, not only for the purpose of illustrating the position of the subjective theory to which I adhere, but much more because I hope these remarks will clarify some common points and points of disagreement between some of the theories, and will more or less facilitate the resolving of some of the difficulties in the reciprocity of the understanding, and approach a possible "reconciliation."

The subjective theory is generally considered to be an extreme position (and in a sense it is) and consequently may seem an inappropriate ground for seeking agreement. However, it is an extreme position only in the sense that it assumes probability in its widest meaning: it does not require restrictions refused by other theories, but ignores the restrictions demanded by these others. For this reason, contrary to what might appear at first sight, the subjective theory gives the proper ground for comparing the different points of view without prejudicing anyone's opinion.

Even the conclusions at which we will arrive, according to the subjective theory, are of such a nature that they approach an agreement. In fact, the opposite posi-

tions and the criticisms on all sides appear to be well founded and are not contradictory, provided that some precision in interpretation is accepted.

2. Nonsubjective and multisubjective problems

Even though the different conceptions and nuances in the field of probability are numberless, we can distinguish, essentially, two opposite tendencies. By the first, probabilistic evaluations and conclusions are possible by using only statistical data or something similarly objective, and any intrusion of probability in the meaning of belief must be rejected. By the other, such a contention is hopeless, and the assignment of *a priori* probability, or something equivalent, is essential in all cases

In spite of all efforts to the contrary, each type of tendency meets with well known difficulties. The first always seems to lack something from which to start and becomes logically defective, while the second vainly tries to justify the *a priori* opinion that it requires.

The subjective theory is, in a sense, the extreme variation of the second alternative; nevertheless, it occupies a position intermediate to the two mentioned above. It does not contend that the opinions about probability are uniquely determined and justifiable. The probability does not correspond to a self proclaimed "rational" belief, but to the effective personal belief of anyone. The difficulty of the second alternative is then avoided, but to achieve this it is necessary to give up the aim of a rational uniquely defined criterion. This paves the way for restoration of the first alternative too, although in a somewhat modified form. From the subjective standpoint, no assertion is possible without *a priori* opinion, but the variety of possible opinions makes problems depending on different opinions interesting. The first alternative becomes useless if conceived as a theory of *nonsubjective* problems of probability but it becomes useful if thought of as a theory of *multisubjective* problems of probability.

A systematic discussion of these questions might follow, as in the afore mentioned paper of Kendall, the logical order given by: Foundations, Direct Theory, Inverse Theory. In this paper the order will be reversed, because the remarks concerning the statistical theory of estimation will quickly give a more concrete insight into these conclusions. About foundations and direct theory I will add only a very few remarks since my opinion on these subjects was widely treated in preceding papers.

3. Statistical theory of estimation and the Bayes' theory

Many controversial discussions have occurred concerning the role and significance of the Bayes' theory on the one hand and that of statistical theory of estimation[1] on the other. We have to say first of all that each theory in itself cannot be attacked, if care is taken to avoid misinterpretations. The first considers the case of known *a priori* probabilities and gives the only true corresponding answer, as Neyman rightly acknowledged. The second is concerned with conclusions which remain true irrespective of the *a priori* probabilities, and no one is entitled to forbid these researches.

[1] For the sake of brevity, we shall always say "statistical theory of estimation," but this must be understood to mean also "statistical theory of testing hypotheses," or, more precisely, the central concept common to these theories.

Most of the discussions arose from misunderstandings. Some authors appear to object (contrary to facts) that the theory of estimation attempts to reach Bayes' results without the necessary premises. On the other hand, some other authors appear to be under the illusion that Bayes' conclusions can be reached without Bayes' premises and attack the theory of estimation and the theory of testing hypotheses because these theories do not substantiate their beliefs. It may still be remarked that these misunderstandings are excusable because of the misleading phraseology used even by those authors who, like Neyman, take the utmost care to avoid misinterpretations, and even after the fortunate introduction of such notions as "decision rule."

For instance, I do not deem the usual expression "to accept Hypothesis H_r" to be proper. The "decision" does not really consist of this "acceptance" but in "*the choice of a definite action* A_r." The connection between the "action" A_r and the "hypothesis" H_r may be very strong, say "the action A_r is that which we would choose if we knew that H_r was the true hypothesis." Nevertheless, this connection cannot turn into an identification.

Misinterpretations aside, the statistical theory of estimation is no doubt correct in itself. This fact, nevertheless, does not also imply that its statements are logically sufficient for the applications for which it is intended. The very frequency of misunderstandings suggests that, for most authors, the statistical theory of estimation is not sufficiently convincing.

I think that the weak point of the theory is precisely the justification that is usually given for the application of certain rules to practical problems. The usual argument is: "by using such a rule, the frequency of wrong decisions will not exceed, in the long run, a given constant α, irrespective of the unknown values of the parameters." This statement is correct. It does not remain so if we consider different kinds of decisions separately. For instance, in the problem of acceptance inspection of manufactured products, it may not be correct to think that the frequency of wrong decisions *in accepting lots* (or in rejecting lots) does not exceed a certain bound. On the contrary, it is quite possible that all acceptance is a wrong decision (obviously in this case the acceptance frequency cannot exceed α).

The reason for this remark is not to refute the assertion quoted above: no one, I hope, asserted it. But, I think, most people will not be satisfied with the weaker correct statement of this theory if they are warned of this essential difference in the stronger condition which cannot be fulfilled except by some help from the Bayes' formula. In fact, consider the rule consisting in accepting every hundredth one of a series of ever defective objects. This rule satisfies the weaker condition (if $\alpha \geqq .01$), but who could accept it as being reasonable?

On the other hand, the weak point of the Bayes' theory is the constant uncertainty concerning the "*a priori* probabilities," their "existence," "knowledge," "evaluation," and "justification."

4. Multisubjective decision theory

If we accept the standpoint of the subjective probability, it seems to be that both the Bayes' and the estimation and testing hypotheses theories become completely satisfactory.

If the "*a priori* probabilities" are not meant to be mysterious objective entities that are known or not, but are expressions of one's own belief, which cannot be unknown to oneself, the validity of Bayes' rule becomes absolutely general.

This theory also gives the only proper foundation for problems or decisions depending on the opinion of a single individual (or of more individuals with the same belief). It is the theory of *unisubjective* reasoning and decisions.

However, the conclusion will not be uniquely determined irrespective of the *a priori* belief of everyone. Under mild restrictions concerning this *a priori* belief, it is nevertheless well known that the discrepancy between these different conclusions becomes smaller and smaller as the width of experience increases. Because of this fact, the choice of such a "decision rule" which is acceptable to many people, in spite of their conflicting *a priori* beliefs, is often not difficult. A general theory of such "decision rules" is, therefore, very important even from the subjective point of view. Its meaning could be summarized as a theory of *multisubjective decisions* (decisions depending on many individuals with different reasonings based on their differing *a priori* opinions). This could become a new concept of estimation theory (instead of the "theory of *nonsubjective* decisions"). The difference does not in any way modify the theory in itself, but only its interpretation in the following sense: In all cases the theory deals with conclusions that remain valid irrespective of certain premises; however, instead of being "irrespective of the values of unknown parameters" (or something like this), it is "irrespective of the opinion one may have concerning the values of those parameters" (or something like this).

This formulation is in no way more narrow than the usual formulation (so that all problems treated hitherto remain unchanged), but may also become wider, so that new problems may arise, the meaning of which will be illustrated by the following examples.

Let $\xi_1, \xi_2, \ldots, \xi_n$ be the distributions of the *a priori* probabilities in Ω, corresponding to the belief of n individuals (1), (2), . . . , (n). (Ω is the class of unknown joint distributions F of $X = \{X_i\}$; $r(F, \delta)$ indicates the *risk* when F is true and a determined decision rule δ is adopted; generally, notation as in Wald's report [3] at the Boulder meeting.) For an individual (h), the best decision rule (rather, the only correct one, according to Wald's position) is the Bayes' rule δ_h corresponding to his own *a priori* opinion ξ_h. Of the two rules δ' and δ'', the first will be preferable for (h), and we will write $\delta' \underset{(h)}{>} \delta''$, if

$$\int_\Omega r(F, \delta')\, d\xi_h < \int_\Omega r(F, \delta'')\, d\xi_h .$$

In this case (of a single individual, or more individuals having a common *a priori* opinion) the role of estimation theory is really not of interest. However, if we consider the n individuals (1), (2), . . . , (n) simultaneously, and we call the rule δ' "better for (1), . . . , (n)" if $\delta' \underset{(h)}{>} \delta''$ for all $h = 1, 2, \ldots, n$ (we then write $\delta' > \delta''$), the problem then arises of determining the "best" decision rule (such that no better rule exists). The above example is intended only as an illustration. Many variations of the above and other questions may clearly be considered. As in the above problem, the adoption of Wald's position is a matter of free decision, the only fact that is essential (according to subjective probability) is that a "rea-

sonable" decision rule depends only on the *a posteriori* distributions in Ω, $\bar{\xi}_1, \bar{\xi}_2, \ldots, \bar{\xi}_n$ corresponding to the *a priori* ones $\xi_1, \xi_2, \ldots, \xi_n$.

Instead of a finite number n of *a priori* distributions ξ_h, we can also consider an infinite set Ξ of such distributions. The usual formulation of the estimation theory corresponds, from this point of view, to the extreme case of the set Ξ containing all possible distributions of the kind considered. The less convincing strength of the weak statement in section 3 depends upon the extent of this assumption and may be more or less improved by imposing more or fewer restrictions on Ξ.

The restriction, that Ξ will contain distributions *of the kind considered*, offers another argument for discussion. This will be taken up later, after a few remarks concerning intermediate standpoints between the two considered above.

5. Current misunderstandings

The intermediate theories do not content themselves with the proper formulation of the statistical theory of estimation, neither do they accept the indispensability of *a priori* probabilities. For the attainment of any strong conclusion, they are, in my opinion, hopeless trials of eclecticism, intended to avoid particular faults or distasteful points of both alternatives without endeavoring to amalgamate their principles in a superior synthesis.

In many theories the authors wish to infer something more using only the premises of the theory of estimation. (In the sense of an "inversion of Bernoulli's theorem.") There are many variations of these fallacious opinions: (i) the mere misinterpretation of the correct Neyman formulation [4], (ii) the recourse to the so called "principle of Cournot" (rejecting the possibility of events with "very small probability"), (iii) the direct adoption of a frequency definition of probability or of an assumption connecting frequency and probability ("empirical law of randomness").

All of these or similar attempts to obtain a theory of probability without starting from *a priori* probabilities or something else concerning one's belief are fundamentally groundless just as are attempts to construct a *perpetuum mobile* by drawing energy from nothing. The differences between these varying procedures are only in the choice of the best expedient for hiding the gap in the logical constructions. Some prefer to analyze this point explicitly in the hope that a way out will be found. Others endeavor to omit these points in the theoretical construction and make them a part of the definitions or of the extra considerations connecting the abstract theory with practical applications. Each is able to find the fault in another's formulation without admitting that the fault is not due to the choice of a particular formulation, but is inherent in the purpose of such a formulation to eliminate from probability theory just that which makes its existence legitimate.

6. A priori probabilities

The theories of the authors supporting the Bayes' position belong to the opposite side of the question. If they do not accept the subjective meaning of *a priori* probabilities, then they are obliged to give artificial answers to the problem of assigning *a priori* probabilities.

Kendall, in the afore mentioned paper, says "situations rarely, if ever, arise, in which there is no knowledge of *a priori* probabilities." This agrees with the opinion that I expressed above, provided the probabilities are understood in the subjective sense. Nevertheless, he adds "but, if such a situation arises, the only possible rule to use is that of Bayes in which all of the possibilities are given the same *a priori* probabilities." This is not only inconsistent with the subjective point of view, but it seems to constitute in itself a very strange idea. On this point, I hope to be in accord with the supporters of the objective statistical theories.

The belief that the *a priori* probabilities are distributed uniformly is a well defined opinion and is just as specific as the belief that these probabilities are distributed in any other perfectly specified manner. Accordingly, there is no reason why the absence of opinion on the distribution of the *a priori* probabilities be taken as equivalent to the opinion that their distribution is of this or that specified form. To be misinformed of the prevailing temperature is not the same as to believe that it is zero degrees; moreover "zero degrees" is not uniquely defined (Celsius or Fahrenheit?) and such arbitrariness occurs in our case too. The above remark is not principally concerned with Kendall, who introduces the idea only in exceptional cases. However, there are authors, with otherwise varying views, to whom this remark generally applies.

It is remarkable that the ideas of the above kind have been recently advanced independently by a number of authors. Carnap's theory (which I know through two papers of G. Tintner and S. F. James [5], [6]) consists substantially in the adoption of equally distributed *a priori* probabilities, for purely formal reasons (syntactical structure of propositions). The theory of Dumas [7] gives particular distribution functions, for random variables, deduced from some invariance considerations.

All of these considerations are, in themselves, very interesting. It is also possible that the properties illustrated in such a spirit will effectively and reasonably induce someone to adopt these opinions in some cases. However, I wish to point out that such a choice is the choice of an opinion, as in all other cases, and not necessarily a consequence of supposed absence of any opinion. This opinion is therefore a subjective one, as is every other, and not one specifically endowed with an objective significance.

7. Constancy of probability and independence

Up to this point we have been dealing with probability without first discussing its meaning. As a result, it was possible to obtain a bird's eye view on the effect that the adoption of the subjective theory has on a number of important questions. In order to obtain a clearer insight into the further consequences of the adoption of the subjective theory it is necessary to discuss some hitherto omitted fields.

First of all, we must take up the thread of discussion interrupted at the end of section 4. We were speaking, there, about the set Ξ of the possible distributions considered in the estimation theory.

To define Ξ we first consider the set Ω of possible distributions of the observable random variables. Then, Ξ is the set of probability distributions defined on Ω. In the simplest example, which is useful to bear in mind in order to avoid unessential

complications, we deal with "an event with unknown constant probability θ." In this case the observable variables refer to outcomes of a sequence of independent trials and the functions F, or F_θ, form a one parameter family Ω, with $0 \leqq \theta \leqq 1$. Any belief concerning *a priori* probabilities corresponds to a distribution on Ω, or, simply, to a distribution on the interval (0, 1) for the "unknown parameter" θ.

What is the meaning of the assumption of constancy and of independence of the probabilities of the trials? This point also seems to be obscure to Kendall, who discusses it in section 50 (see also sections 19 and 29–32) of the mentioned paper. In fact, I never have understood what meaning this assertion could have for a frequentist, who gives significance to the probability for only a collective of phenomena, while here it is to be applied to the single trials. The answer of the subjective theory is unambiguous; I do not know if it would appear satisfactory to Kendall, who was evidently not acquainted with it and considered the question to be almost totally unsolved.

According to the subjective theory there is no constancy of probability and no independence in this situation, but only "equivalence" (or "exchangeability"). It is this property that remains valid for a linear combination

$$F = \int F_\theta d\xi\ (\theta)$$

if independence holds for every F_θ. I will not repeat here what I have repeatedly exposed on this subject. For the mathematical theory see my lectures [8] at the Institut Poincaré; for the logical significance see a paper in *Dialectica* [9].

And now, what does "independence" mean? This is also a question which Kendall (sections 29–32) acknowledges as unsatisfactorily answered in the expositions that he knows. The answer of the subjective theory is unambiguous, and does not, I think, give rise to the doubts and objections of Kendall. The evaluation of a subjective probability concerns only a single trial. The trials are said to be "independent" *in a probability opinion* if the value given to the conditional probability of a trial remains unchanged for any assigned result of the other trial. This notion is therefore also a subjective one, depending on the chosen opinion.

8. Ideas of von Neumann and Morgenstern, Pólya, Kendall, and Savage

How can the subjective theory give procedures for the evaluation of a probability if it is so vaguely conceived as a mere "degree of belief"? Also, how can the principles of the theory of probability be rigorously established? These are questions which I have repeatedly treated. I will only remark here about some connections with independent developments of authors such as Pólya [10] and von Neumann [11].

For the measurement of the degree of belief, or probability, felt by a given individual, the subjective theory considers the conditions of a bet which this individual would accept (under proper circumstances). It is remarkable that in introducing probabilities into the foundations of economics, von Neumann and Morgenstern were led to considerations very similar to those in the subjective theory of probability, underlying the restrictions necessary and sufficient for avoiding inconsistencies among conditions of betting.

In a different way, the subjective theory can be founded by a criterion of mere comparison, in qualitative form, and this is the concept of the theory of plausible inference put forward by Pólya. More exactly, as I have shown in a report to the Rome Meeting of the Italian Society of the Advancement of Science in 1949, Pólya put forth a new idea of a more general type of logic, which contains the subjective theory of probability.

The inductive reasoning, illustrated by Pólya, is exactly the subject of the subjective theory of probability. Only on one point might there be disagreement. In his introductory remarks, Pólya distinguishes as different two meanings of probability, the statistical one connected with frequency and the other in the sense treated here. I do not know whether this distinction in Pólya's writings is simply due to the actual existence of statistical theories based only on the concept of frequency or whether it is his conviction that these two aspects of probability are unavoidably the objects of two essentially different theories.[2]

This second alternative is not in accord with my opinion, which coincides, in this respect, with that of Kendall. Contrary to the common opinion that "the difference between the frequentists and the nonfrequentists are largely due to the difference of the domains which they purport to cover," Kendall asserts that "this is not so," that "they differ because they approach the same problem differently, not because they deal with different problems." I am in perfect accord with him when he asserts that the law of formation of our opinions is essentially always the same whether the problem treated is of a statistical nature or not. This law is precisely the plausible inference of Pólya, that is, in the quantitative formulation, Bayes' rule.

Nevertheless, it seems to me, that, at this point Kendall deviates from the consistent application of the adopted point of view. In fact, he deems it necessary to make recourse to an additional axiom connecting probability and frequency based on empirical verifications. The plausible inference gives the most general justification for the inductive reasoning, which applies also in the particular case of statistical stability of a frequency. The attempt to justify the principle, by supposing it to be demonstrated under some other formulation, is a detour which prevents us from defining the question carefully.

As far as it is possible to judge from a brief summary, a completely consistent formulation of the principles of subjective theory appears to be contained in the paper [2] by L. J. Savage, already referred to.

It is my conviction that, by adopting the subjective theory of probability as outlined in the present paper, all the difficulties encountered in other theories can be answered naturally, unambiguously and coherently. Therefore, I believe that it may be useful for everyone to become acquainted with this way of conceiving probability and its problems, no matter whether the subjective standpoint is finally accepted or not.

[2] At the Symposium and at the Mathematical Congress of Cambridge, Mass., I had the opportunity of learning Professor Pólya's opinions on this subject; essentially he is interested in collecting examples of inductive reasoning, but he prefers to maintain an agnostical position between the theoretical constructions concerning this subject.

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